A Challenge to Handwriting Experts and An Answer to Their Critics

Formerly: Appendix A
"Texas du Pont/Daubert..."
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APPENDIX A: ANSWERS TO CRITICS OF HANDWRITING EXPERTISE.
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This appendix, intended for both attorneys and document examiners, replies to criticisms raised against handwriting expertise by Risinger, et al., and others. First, however, let it be clearly understood that I hold that there are inadequate examiners of handwriting and documents and some who give indication of being nothing more than available hirelings, skilled at providing slick and plausible "evidence" why the genuine is false and the false genuine. This is a defense of the competent and ethical examiner against mistaken and illogical attacks. Since attacks in academic legal papers are polemics under disguise of objective and disinterested academic scholarship, my remarks are honest and up-front polemics based on as accurate and thorough scholarly research as my time and personal resources permit. Repulsed by the snide and underhanded quip as by the cutesy put-down in titles to papers and parts of papers, I prefer an honest casting of the gauntlet

A. BACKGROUND.

During the early part of the Twentieth Century, American document examiners gradually overcame the judicial, but reasonable, bias of past ages against handwriting expert evidence. They did so by directly satisfying the basis of the bias: unreliability of some of such evidence which was not always rooted in experimental and/or common experiential findings. In America, Albert S. Osborn led the way with the close cooperation of John Wigmore. Subsequent to the Second World War, handwriting experts began forgetting the intimate knowledge of the underlying anatomical and physiological data and neglecting the rigid methodology by which the classical examiners had won for them admissibility expressed in both statutory and case law. Government handwriting experts began thinking that the only way to learn was from government courses, eventually taught by those who had only learned from government courses. However, in the 1970s competency studies targeting local, state and Federal government laboratories exposed the lack of mastery. See: Peterson, Joseph L., et al. Crime laboratory proficiency testing research program. Washington, DC, National Institute of Law Enforcement and Criminal Justice, Law Enforcement Assistance Administration., US Dept. of Justice, Oct. 1978.

How to prove substance in a world with too much expert fluff? Those considering themselves the leading lights obtained Federal grant money to establish a certification program. Taking the name American Board of Forensic Document Examiners (ABFDE), they certified themselves, simply because they were the ones who got the grant money. It is called grandfathering the certification, which in reality is only an empty self-crowning absent any objective testing for competence or knowledge. But it did the trick, as even courts of law thought the self-bestowed honor represented tested reality. For example, see State v Livanos, 725 P2 505 (AZ Ap Div 1 1986), where the Court of Appeals upheld the trial court's not allowing the handwriting expert for Defendant to testify. Among other things, he was not certified by ABFDE and was a member of WADE, whose admission standards were very informal. But what could be more informal than ABFDE's grandfathering of the original members? Their present membership does not represent even a plurality among practicing and court qualified document examiners in North America. Myths are wondrous things, often more powerful than reality.

In more than 20 years, has ABFDE enriched the profession with ever increasing numbers of ever more qualified practitioners? Not according to hard numbers provided by an author who does not scrimp on adulation for them: Andre A. Moenssens, "Handwriting Identification Evidence in the Post-Daubert World." 66 UMKC Law Review, 251-343 (Winter 1997). At page 335 are given dismal numbers for the effectiveness, and maybe the honesty, of ABFDE certification. Starting in the 1970's, they gave away to themselves at least 180 certificates. Subsequently, over approximately a twenty-year period, according to Moenssens, they granted only about a hundred certificates on purportedly proven merit. That is an average of approximately five a year. With 168 active diplomates in 1997, they retrogressed from the 180-190 active at the beginning. Was your Federal tax money, which set up this little private QDE club, effectively or ineffectively invested?

It was long a picnic in the litigational park. Then entered the ants: Risinger and company. The picnic has never been the same.

B. ENTER THE ANTI-EXPERT EXPERTS: INEXPERTISE ON EXPERTISE.

1. Risinger and Company attack.

An even more mythical myth challenged the myth of ABFDE and its friends in the publication: D. Michael Risinger, et al., "Exorcism of ignorance as a proxy for rational knowledge; the lessons of handwriting identification expertise." 137 University of Pennsylvania Law Review, 731-92 (Jan. 1989).

The authors of this paper attempted to invalidate handwriting identification completely. It is an excellent armory for attacking the handwriting expert, provided you realize the bias, invalidity, historical inaccuracies, technical errors and illogical nature of their argument. And provided the expert being attacked is even more unknowledgeable of reality than the authors the attacker depends on. In a follow-up article, two of the authors heroically hang onto their misconceptions and false assertions. If they had been law students rather than law professors, they would have received very low marks for both papers, due to substandard research, misstatements of fact and law, and atrocious logic. But their second title was almost as catchy as the first: D. Michael Risinger and Michael J. Saks., "Science and nonscience in the courts. Daubert meets handwriting identification expertise." 82 Iowa Law Review, 21-74 (October 1996).

They repeat their belief that all expert testimony must meet all seven criteria given in Daubert v Merrell Dow Pharmaceuticals, Inc., Schuller v Merrell Dow Pharmaceuticals, Inc., 727 FS 570 (S. D. CA 1989); affirmed, 951 F.2d 1128 (9 Cir 1991); vacated and remanded, 125 L Ed2 469, 113 S Ct 2786 (1993); affirmed, 43 F3 1311 (9 Cir 1995); cert. denied, 116 S Ct 189, 133 L Ed2 126 (1995). The only problem with that is that the Supreme Court itself said such is not the case. It must meet those or any other reasonable set of reliability criteria applicable in the particular case for the particular expertise. See Kumho Tire Co., Ltd., et al. v Carmichael, et al., 526 US 137, 143 L Ed2 238, 119 S Ct 1167 (1999), reversing Carmichael v Samyang Tire, Inc., 131 F3 1433 (11 Cir 1997).

2. Experts without an expertise.

More telling is that those, who testify to the purportedly scientific theory of Risinger and his fellow travelers concerning expert evidence, have not bothered themselves to meet what they

claim are the essential criteria for admissibility. For example, none of them underwent competency testing as expert witnesses on the scientific reliability of their claimed ability to criticize other expert witnesses; none have published peer reviewed papers on their theories; none have based such theories on statistically analyzed empirical research. To my knowledge, after much reading in their writings and court cases, there is not even a name for their specialized expertise except my christening of them as "anti-expert experts." Nor does it seem that any attorney whose client was being victimized by their self-proclaimed superiority to others has demanded that they meet their own vaunted standards.

3. Replies to "Exorcism of ignorance."

Here is a brief sampling of their fallacies from their paper, "Exorcism of Ignorance," after which we will look at their nonsense in "Science and nonscience." You will need these if your expert is attacked by such groundless challenges. However, not all errors are considered, since one need only jabber effortlessly and repetitiously to multiply error, while reply to each error so multiplied takes time and effort.

(a) Page 733, "No court anywhere had ever explicitly considered and passed on its [handwriting expertise] claims of validity."

REPLY: Only one case is needed to refute the universality asserted, and a very old one is deliberately chosen. The case Hanriot and al. v Sherwood and al., 82 VA 1 (1884), considered the objective reliability of handwriting expertise and so ruled that the trial court had erred in not admitting it. At page 8: "Experts have for years asserted the possibility of investigating handwriting upon scientific principles, and the courts have, consequently, admitted such persons to testify in cases of disputed handwriting. It may be asserted that experiment and observation have disclosed the fact that there are certain general principles which may be relied upon in questions pertaining to the genuineness of handwriting." [Emphasis in original.] The Court then goes on to elaborate on this idea, concluding at pages 15-16: "[A]nd while we find that formerly there was a great conflict of authority on the subject of comparison of handwriting, the advance of science and the growth of business and the necessities of the people have led to changes and modifications of the rule in so many States that now the great preponderance of authority appears to be on one side." If that is not a consideration of objective reliability, then there has never been such consideration for any expertise, nor will there ever be.

This case also answers other misconceptions of the "Exorcism" authors. It gives a history of the expertise which exposes errors in the historical assertions of the authors of "Exorcism" and of other anti-expert experts. For example, at page 6, speaking of persons of skill in handwriting comparison, such testimony "has been used in arriving at correct conclusions in courts of justice from the earliest periods." Roman law permitted it. Then at page 10: "In England a comparison of handwriting placed in juxtaposition has always been permitted in the ecclesiastical courts."

(b) At page 733: "There exists almost no studies of its claims in any academic literature."

COMMENT: In this monograph studies are cited which are directly on the task taken as a whole. However, if we isolated specific aspects, citations would more than quadruple the size of this paper. For example, effects of some illnesses and drugs on handwriting are extensively researched and reported in the medical literature, all of which validate research reported in QDE literature and support what knowledgeable and competent handwriting experts do when confronted with writings made in such circumstances.

(c) At page 740, et seq., they discuss Fred E. Imbau, "Lay witness identification of handwriting; an experiment." 34 Illinois Law Review, 433-43 (December 1939). They make speculative statements as bases for discounting it, such as at page 741: "Thus, Imbau's sample of document examiners was small and skewed by a self selection that probably eliminated those who were insecure about putting their abilities to a test...." [Emphases added.] They list what they consider methodological flaws.

COMMENT: These authors regularly, in both their published writings and trial testimonies, ultimately base their opinions on speculative assumptions which they never feel obliged to prove, then they make definite and undoubted "findings." Yet they are taken seriously. As to purported methodological flaws, they never make a methodological analysis of any writing that agrees with their opinions, presumably because to agree with them is the ultimate mark of one's reliability. This practice is simply academic duplicity.

(d) At page 744, et seq., they consider the series of proficiency tests beginning with Joseph L. Peterson, et al., Crime lab. proficiency testing research program. Washington, DC, National Institute of Law Enforcement and Criminal Justice, Law Enforcement Assistance Administration., US Dept. of Justice, Oct. 1978. They consider an "inconclusive" opinion as an incorrect opinion, or at least as not correct.

COMMENT: It takes a true and honest expert to recognize the limitations of one's own expertise and of one's own skill so that one would rather say nothing than say the incorrect thing. This modesty is a hall mark of both personal integrity and professional competence, a modesty which the anti-expert experts not only seem incapable of appreciating but also seem not to possess in the least bit.

(e) At page 751 et seq., they give a most unhistorical history of handwriting expertise in court. At pages 755 and 756 they speak of Folkes v Chadd, 3 Doug 157, 99 ER 589 (1782), affirmed 3 Doug K.B. 340, 99 ER 686 (1783), and of Lord Kenyon's cases.

COMMENT: They speak of Folkes as "the first recorded opinion addressing the propriety of anything resembling expert testimony in any context...." Did they fail to note that this case stated that expert testimony was routinely admitted by courts? Though they state that some of Lord Kenyon's cases involved inspectors of franks, they erroneously assert elsewhere that there were no handwriting experts until after enactment of the statute permitting such evidence. By and large, their "history" is a report of their interpretations of events as if such were the events themselves, rather than a report of events that they afterwards interpret.

(f) At page 764, et seq., they discuss "The Crusade of Respect for Handwriting Expertise."

COMMENT: The title of the section shows inherent bias and mockery. If one reads the segment critically, one feels these are midgets kicking at the shins of giants who are now dead and cannot reply to the impertinence. Their one claim to admiration is that they are iconoclasts, meaning they discovered they can only destroy, and so they make destruction a virtue and manufacture a rationale for an activity that has no inherent merit. When one considers that their entire thesis is built on mere assumption, namely that the Daubert criteria have any basis in empirical reality other than group "scientific" consent, meaning general acceptance of these common myths that Karl Popper premised on nothing other than his own subjective pronouncements of what makes science to be science, then one will not take their belief system seriously while recognizing the very serious damage it does.

4. Michael Risinger and Michael J Saks, "Science and nonscience in the courts. Daubert meets handwriting identification expertise." 82 *Iowa Law Review,* 21-74 (October 1996)

(a) At pages 22-23 they say development "of a system of handwriting expertise appear to have started in Italy and France in the seventeenth century...." They cite Camillo Baldi's treatise as "the earliest in this line." They then claim it did not exist "in the English speaking world" until "lawyers persuaded the courts to accept the idea that such an expertise might exist." They say handwriting expertise was the first forensic expertise allowed in court.

COMMENT: Roman law permitted the expertise and it was systematized, as Quintillion sets forth. See: Royston J. Packard, "Handwriting at court." 2 Criminal Law Quarterly (Canada), 37-40 (May 1959). Baldi's treatise was what today we would consider a combination of graphology and linguistics, for it had nothing in it regarding forensic handwriting identification. See: Camillo Baldi. Discourse on Indications in missive letters. (Tratatus de signis ex epistolis.) Translated into Latin by Ietro Velli, 1664. Translated into English by Robert E. Backman, 1959. Greenfield, MA, HARL, 1959, 1965, 1984. The expertise was already permitted in English courts to some degree long before the authors claim, as shown elsewhere in this paper, and there was a coterie of experts preexisting their admission in court. It is simply preposterous that any court rules admissible a proffer of evidence which is not currently physically existent and actually presented to the court. Risinger and company come the closest ever of being ruled admissible without presenting any currently existing, definable and testable expertise. As usual, getting their history wrong, what may be the first reported English case of expert evidence going to a jury is Alsop against Bowtrell, 17 Jac 1; Cro Jac 541; 79 ER 464 (1620). Expert medical evidence was accepted that a child born 5 January 1611 to a widow of a man who died 23 March 1610 "might well be" his child carried the day for his widow who had been badly abused by her father-in-law. This case refers to earlier expert medical evidence on the same issue.

(b) At page 30, they say: "Finally, and importantly for this Article, footnote eleven makes clear that the requirements of FRE 702 set out in Daubert apply to all scientific evidence, not just to 'novel' evidence."

COMMENT: Footnote 11 of Daubert v Merrell Dow Pharmaceuticals, Inc., 125 L Ed2 469, 113 S Ct 2786, 509 US 579 (1993), appearing at 125 L Ed2 469, page 482, reads: "Although the Frye decision itself focused exclusively on 'novel' scientific techniques, we do not read the requirements of Rule 702 to apply specially or exclusively to unconventional evidence. Of course, well established propositions are less likely to be challenged than those that are novel, and they are more handily defended. Indeed, theories that are so firmly established as to have attained the status of scientific law, such as the laws of thermodynamics, properly are subject to judicial notice under Federal Rule of Evidence 201." Later these authors will pretend they never read Daubert as applying only to scientific expert testimony and not to all expert testimony, and they will take to task courts which had the same clear impression of the U.S. Supreme Court's words. But they ignore the clear implication that the Court considered unlikely the challenge to well established expert evidence.

(c) At page 32, speaking of rulings regarding admissibility: "Velasquez is somewhat more problematic than Starzecpyzel because it is more ambiguous."

COMMENT: See discussion of Velasquez in Appendix B where it is shown that the Velasquez Court explicitly rejected the proposition that Ms. Bonjour was not offering scientifically reliable evidence. This shows that these people's representations of what others have said are not to be relied on since they skew it to subserve their own mental outlook.

(d) At page 35: "[C]ourts will at least have to develop and enforce standards designed to maximize likely dependability and eliminate the grossly suggestive methods by which handwriting identification problems sometimes are presented to examiners for evaluation."

COMMENT: They absolve themselves from having to demonstrate "grossly suggestive methods," but from my personal experience such do happen, even to the point of stating precisely and explicitly what the desired conclusion is. However, it is often a valiant and futile struggle to keep a client from asserting such. The best way to head off such assertions is to explain that everything said to the expert is discoverable at trial, and knowing the client's position will surely make any opinion, however honest, suspect. Self-interest is a strong motivation to do the right thing that one is otherwise disinclined to do.

(e) At page 36: "The major source of individuation is what we may call a tagged residue." And they later speak of a "residual probability of error" in any process of identification.

COMMENT: What they then explain that "tagged residue" means has nothing to do with either a tag or a residue, at least in handwriting identification. It might in ink analysis or explosives investigations; but, in a thing which is the physical/mechanical effect of a behavioral cause in a habitual act, it is precisely the habitual and behavioral effects that will identify the cause. Such is handwriting, particularly a signature. As to the probability of error, there must well be some reasonable basis in this identification for crediting the probability. They also miss the point that identification, as to say who it was, and elimination, as to say who it was not, are the result of two different standards, the latter much more easily permitting a definite finding. Again, a true and honest expert, an ethically and scientifically reliable expert, knows when to withhold an opinion. I have given expert opinions, indeed definite opinions, that the writing in question is of such a nature that it cannot be identified as to its maker. See: Jacqueline Joseph, "The unidentifiable handwriting; an anonymous note case." 20 Journal of the National Association of Document Examiners, 20:1-5 (Spring 1997.)

(f) On page 39, et seq., they discuss "The Claimed Principles of Handwriting Identification."

COMMENT: They are simply as ignorant of the correct principles just as the handwriting incompetents and hirelings are. They do not realize that what they describe are "experts" who could be impeached, if not disqualified, for not following the standards or simply for defying common sense. Their practice as anti-expert experts is thus shown to be most inept and maybe unethical, because in their ignorance and presumption they fail to assist the client attorney to impeach this opposing expert and to employ the expertise to exonerate the innocent client, if the client be innocent. Attorneys who hire these anti-expert experts might well be giving inadequate assistance of counsel because of their gross misconceptions and ignorance, as well as their sabotaging the employment of an effective expert for the client. If as a result an innocent client is convicted, might it also be legal malpractice and expert malpractice?

(g) A general fallacy which appears in several papers of this kind is that handwriting expertise only has courts of law as clients.

COMMENT: The expertise has many non-litigational uses, such as in the art market and in the manuscript trade. Paleographers use it to identify which ancient scribes made which writings and to prove historical forgeries. Many clients of document examiners have no intention of taking the opinion to court. Other disciplines also apply some aspects of the expertise. Taken literally, their claim that courts are the only clients means that examiners are hired and paid by courts themselves, a thing that happens very rarely. So they are not only factually wrong but are also inept as men of letters.

(h) Repeated in this paper and others is the honest confession that Risinger, et al., could find no scientific papers on handwriting expertise in academic libraries; they conclude, therefore, that none exist anywhere.

COMMENT: The reader by now, as well as later, has become familiar with the reality which marks as woefully inadequate both their efforts and their competence in researching the scientific literature, and thus suggests that both leave quite a bit to be desired. The few court cases cited herein show that as law professors they ought to bone up on basic skills in legal research. Any literate citizen can learn to find a thing in a university or public law library if the thing be sufficiently desired and the search be sufficiently diligent.

There are several other fallacies in the several treatises by this same trio, but the above two papers illustrate that we ought to investigate such bald assertions and think carefully before blindly and dumbly accepting them as the final word on the matter. It is good that courts and attorneys are now on their guard not to take the word of handwriting experts as received truth. But that same healthy skepticism must be extended to all experts and authors, including the monograph you are now reading. It is only as good as the reasons for its statements and as the quality of its logic. Require no less of any expert testifying against your client—or against your expert!

5. An academic herd instinct.

Other authors have offered criticisms of handwriting expertise (or the same authors at times), but they give much the same false arguments as Risinger and company. However, I will strive to discover something original in their criticism, a most difficult thing to discover on the whole. It is amazing how academics so readily adopt a herd instinct in what is presented as scholarly thought, provided it gives one professional stature and more publications to bolster one's career, all with minimal intellectual labor. Going in no particular order, first to be considered is Jeffrey M. Schumm, "Precious little guidance to the 'Gatekeeper' regarding admissibility of nonscientific evidence: an analysis of Kumho Tire Co. v. Carmichael." [Section on handwriting analysis in Federal courts, p. 883-8.] 27 Florida State University Law Review, 865-95 (Summer 2000).

(a) Jeffry M. Schumm.

The author at page 885 states that the District Court's opinion in Hines "remains by far the most enlightening post-Kumho opinion regarding the admissibility of handwriting analysis experts." Other decisions equally analytical but going contrary to the author's views were either not seen or dismissed as insufficiently enlightening. One gets the nagging feeling that most legal "scholars" are highly selective in their sources and fundamentally very focused as to what is pertinent in the sources selected. Their reportorial style is to be interpretative and present that as journalistic objectivity. Schumm's piece is an excellent example of this tendency in the alleged "disinterested academic."

(b) D. Michael Risinger and Michael J. Saks. "A house with no foundation. Forensic science needs to build a base of rigorous research to establish its reliability." *Issues in Science and Technology Online* (Fall 2003)

That their "recommended reading" includes only themselves and others of like mentality speaks eloquently of their "scientific" self-assuredness and degree of objectivity. In summary, they take others to task for having no adequately reliable bases to support their expertise, precisely what the authors and their fellow anti-expert experts totally lack in support of their own alleged expertise, whatever it might be labeled. It is suggested that they see themselves in a mirror darkly and think they see others.

(c) D. Michael Risinger. "Defining the 'task at hand': Non-science forensic science after *Kumho Tire Co. v Carmichael.*" 57 *Washington and Lee Law Review,* 767-800 (Summer 2000)

This is drivel similar to other writings by the same author and his colleagues.

(d) Michael J. Saks. "Merlin and Solomon: Lessons from the law's formative encounters with forensic identification science." 49 *Hastings Law Journal,* 1069-1141 (April 1998)

The first parts of this paper, through page 1094, are sort of right at best, but it is mostly the erudite work of a spin master. The most apt term to describe the scholarship in the later parts of this paper is "cutesy." Just a few of the headings will turn off a serious minded reader: "Handwriting Identification: Heads, the Proponent Wins; Tails, the Opponent Loses," "Fingerprints: Vouched for by Dr. Twain and God." [He has no qualms about mocking anyone, courts of law, law makers and even God.] "Toolmarks: Reversals without Reasons." "Voiceprint: Judicial Cacophony." It is recommended that one never read him and his colleagues closely. Indeed, the interminable boredom of his flippancy and excessive verbiage should justly condemn his mass of published and testimonial musings to literary oblivion.

(e) David L. Faigman, *Legal alchemy; the use and misuse of science in the law.* New York, W. H. Freeman & Co., 1999.

Faigman is a professor at Hastings School of the Law, a unit of University of California, in San Francisco. I am most appreciative of the staff of the library there who have been so gracious and helpful in my researches over the years. This book echos the by now highly uncreative criticisms of handwriting expertise. Since it offers nothing of originality, except maybe the title, I shall share a story which more than anything else illustrates the attitude of the professional, or at least habitual, cynic and sceptic.

When invited to participate in a class at Hastings on admissibility of evidence, I was asked to make a few remarks to the class afterwards. Professor Faigman had played the anti-expert expert, and I the handwriting expert. In my remarks I stated that another person's ignorance is not the measure of my knowledge. I directly stated that his premises were incorrect and he misunderstood what he criticized. His reply, as the class broke, was that I was responsible for enlightening him. Before I could offer enlightenment or point out that he wrote his book without ever asking for enlightenment of me, he quickly exited the room.

It reminded me of how Pilate replied to Christ's words: "I have come to testify to the truth, and whoever is of the truth hears my words." Pilate's famous reply was in two parts. First: "What is truth?" Second: A quick exit before Christ had a chance to answer. After all, if Pilate had

adverted to any truth of that particular occasion, he could not have later pretended that he who alone could order the crucifixion had no responsibility for it. Maybe that is why one devoted to being a cynic and sceptic must always find an excuse to discount any contrary evidence or, if it cannot be discounted, put enough twist and spin on it so that it seems to support its very contrary.

(f) Andre A. Moenssens, "Handwriting identification evidence in the Post-*Daubert* World." 66 *University of Kansas City Law Review,* 252-343 (Winter 1997)

This is a defense of handwriting experts, but it attacks a segment of the field and falsely suggests that this segment is responsible for the problems the critics make much of. I wrote a 19-page assessment in 13 point type of this 92-page article in decidedly smaller type, but Professor Moenssens told me he did not read the assessment because it was too long. Here are some pertinent points:

• About half his text is in footnotes, an epidemic behavior in law articles. If the footnotes are of importance to the theme, why not integrate them into the text? If not, why not drop them? Legal writers do not seem to believe in saying a thing once but well.

• The author does not offer precise definitions of terms discussed, another characteristic failure of legal authors. Yet law is known for precision of definition.

• Graphologists are blamed for the poor showing in proficiency tests reported by Peterson, having "invaded" the field. But no graphologist was included in those studies.

• In footnote 17 at page 260, court cases are interpreted as damning graphologists as document examiners. For example, in Hooten v State, 492 S2 948 (MS 1986), the dissenting opinion is given much shrift as to why an expert with graphoanalsyis background ought to have been excluded, but it is not pointed out that the dissenter fully supported the same expert in Culbreath v Johnson, 427 S2 705 (MS 1983). Law professors ought to be ashamed that they do either slovenly or partial and selective legal research in case law to support their pronouncements.

• The author tells how he castigated IGAS members as being unethical for testifying they had received instruction from him, yet the outlines of the course he gave that organization were for a full week, covering multiple topics in depth, and ending with a comprehensive written test. Experts attending presentations of even half day or less, and with no testing to see whether they learned anything, list these under "education and training" on their CVs.

• Generally, apart from these and other shortcoming, the author does a commendable job of defending the expertise, and the paper is recommended to your study.

• The critics jump all over Professor Moenssens' more glaring faults in their reply: D. Michael Risinger, et al., "Brave new 'Post-Daubert World.' A reply to Professor Moenssens." 29 Seton Hall Law Review, 405-90 (1998). For example, his dividing of handwriting examiners into two camps, but they miss the point that the Moenssens-preferred camp were the ones doing shamefully poor on the proficiency tests. However, they take him to task for proposing a separate set of criteria for such areas of expertise as handwriting identification, a proposal fully validated by Kumho. I have seen no paper by the critics where they go back and credit Professor Moenssens for this astuteness. They do go on a long time explaining what "science" means in Daubert and among modern scientists of an acceptable stripe. I dare any reader to understand what they mean no matter how often read and studied their explanation is. It comes down to having been accepted into the club, or at least garner the approval, of those who have access to

the grant money and high academic appointments. Visit the Web page with the CV of one of their friends, George E. Stelmach. The man's testimony in Starzecpyzel confesses he does not understand the dynamics of handwriting, but he sure knows how to gather in the grant money. Can one man really do all the research and write all the things he credits to himself in any given year? I suspect it is like the Kings of the Ancient Near East: Control the money, have others do the fighting and building, but put one's own name only on it all. Such were the bases why Stelmach said he considered himself a scientist. If that be science, may God preserve all honest people from the contagion.

(g) Reni Gertner, "Criminal Defense Lawyers Mount New Attacks on Forensic Evidence." *Lawyers Weekly Archive,* Dec. 11, 2000.

This news report considers the first specific expertise in "Successful Handwriting Challenges." It quotes Saks and Denbeaux, but no one else, much less a legitimate handwriting expert. Assuming the journalist quoted the two correctly (and anyone involved in events later reported by journalists knows the high likelihood of misreporting), the two give incorrect information which could have easily been confirmed in 2000. The reporter apparently did not have the conscientiousness to verify the purported information. I consider this article because it shows how falsehood can multiply itself, thus creating more "evidence" from "authoritative" sources that it is really true.

Saks is quoted as saying: "Handwriting identification evidence has, without exception, gotten thrown out or [there has been] limits placed on what expert can say." The cases considered in Appendix B include 29 wherein there was neither limitation on nor exclusion of the handwriting identification evidence. Three of these had Denbeaux, Saks' buddy of "Exorcism" infamy, excluded by the trial judge. Unfortunately, in U.S. v Velasquez, the Appeal Court said he had the barest qualifications to testify. In U.S. v Paul and U.S. v Ruth the Appeal Courts had much better sense, leaving him excluded because of his abysmal lack of qualifications and/or reliability and/or helpfulness. Saks is a professor of the law and a self-styled research expert, yet he seemed unaware of case law contrary to his universal assertion and of how his buddy got the judicial boot. How do these guys manage to avoid so much reality?

Regarding Denbeaux Gertner writes: "In the past two years, says Denbeaux, he has seen 'somewhere between three and five hung juries and at least that number of not guilty verdicts' in cases where he has testified." Funny he seemed to have no balancing recall of cases where his client got convicted or he got disqualified by the trial judge. He is another professor of law that seems to have been absent the day in his freshman year at law school they talked about how to find all pertinent case law. The "past two years" would probably cover both U.S. v Paul and U.S. v Ruth. I wonder about these law professors playing at expert witness. Do they collect full pay from their universities for when they are off earning pay as retained witnesses? And why are they called "professors" when being professors, that is teaching, does not seem to be the activity most consuming of their time?

The major lesson from the Gertner article is that multiplication of reports saying the same thing is not the same as greater weight of evidence. It is mere bolstering since the sole source is the original trio who wrote the "Exorcism" piece. No amount of echo equates to another original noise.

6. Jennifer L. Mnookin, "Scripting expertise: The history of handwriting identification evidence and the judicial construction of reliability." 87 *Virginia Law Review*, 1723-1845 (December 2001)

This paper is given fuller treatment because it is such a handy compendium of so many fallacies and factual errors. The case law discussion is not exactly impeccable, but then it is by another law professor, and that seems to be a reasonable explanation why an author can have so much legal data so wrong and be so substandard in legal and related research.

(a) First, this author is even more insufferably repetitious than the others reviewed. I suspect that, being professors, their experience compels them to repeat and repeat and repeat lessons. Students are prone to daydreaming at the best of times, and, if these professors are as boring in lecture as they are in text, their students might only hear one in ten of the repetitions of any given idea in any given class. I highly recommend all these legal papers as particularly pertinent reading during Lent; their perusal, and even more their penetrating study, is that penitential a pursuit. The advantage of reading this paper is that one need invest very little intelligence because the ideas, that she deems worthy of such repetition and of 100 more pages than needed to say them, are quite simple to the point of being simplistic.

(b) One idea is that expert testimony is either the expert educating the fact finder or the fact finder deferentially accepting the expert's word, and this creates a tension, the nature of which is never defined, but it is suggested to be virtually eternal without any alternative. That is not an original idea with this author, because she cites others who wallowed in it previously, that is, also wrote interminably long papers to say the same simplistically small thought. In the midst of saying this simplistic thing she trips across the answer handwriting experts gave so that one rises above this seeming tension. More about that later.

(c) A second idea is that handwriting experts, none of them, ever had any legitimacy or any substance. They did some kind of conspiratorial com game, a 100-year P.R. program. She does not use those words (that would be too efficient, direct and unrepetitious), but the sum and substance of her protracted "history" is assertion that only her thesis is the truth. Yet she unwittingly portrays the exact opposite, but reality is interpreted as really being not what it is.

(d) A third idea is that courts gave clues to handwriting experts how they could insinuate themselves into trials as admissible experts, then experts following such clues from judges conned the judges into believing handwriting experts met the courts' expectations, consequently judges decided handwriting experts were top notch experts, while lawyers finally realized they could use these charlatans to win cases. Soon, everyone believed the myth of handwriting expertise. That is the main theme of the paper and the explanation of the catchy, but rather obtuse, title.

(e) There is really no fourth idea. However, there are two handy tools for "proving" these ideas to be true. Tool number one: Wise cracks! These wise cracks come out mostly as snide insults in the guise of reporting historical facts. Tool number two: Whenever anything proves the opposite of her thesis, it is shown to prove the opposite of its own reality, so that her a priori commitment to her belief system triumphs over all a posteriori revelation of reality. Confused? I hope you are intelligent and logical enough to be, but despair not. Examples will illustrate this very clever "jury argument" by an attorney defending a defenseless case. To put the two tools into one technique: Partisan interpretation of facts is given as if it were objective reporting of

history. She is a legal spin-master, the best I have read so far. Some specifics are now offered, so you will be happy you never suffered through this paper but will have empathy for me who did.

(f) At page 1727 she restates the myth that handwriting expertise was invented "for use in the legal arena." Never mind that she later notes that the classical examiners were called because they had the expertise from teaching penmanship, practicing calligraphy, publishing about handwriting, or other relevant practice. As noted elsewhere in this paper the examiners of franks were professional handwriting experts employed without reference to courts of law. On the same page and on to 1728 she says: "While history cannot provide straightforward answers to the current dilemmas about admissibility...." Her anything but straightforward history makes sausage of history and gives no reliable answers.

(g) At pages 1728-1729: "The historical story also suggests that experts exhibit a surprising willingness to shape their testimony explicitly to comport with judicial ideas about what is persuasive." That description fits perfectly but one group of alleged experts, Saks and his like. They manufactured their unheard of expertise out of whole cloth and a misinterpretation of Daubert case law as shown in both Appendices A and B.

(h) At page 1729: "The courtroom does not just import reliable evidence, but also creates it." If the court creates evidence it is anything but reliable. Her thesis demands that judges not only act against law but be highly unethical. Or maybe just plain stupid. Maybe it is because I am of what she conceives as a low class of expert witness, but I think judges are historically on the whole not so silly as she portrays them.

(I) At page 1730 and before and after, she beats the dead horse of "education versus deference" model of expert testimony to further death. However, as she notes later, Osborn and other handwriting experts gave the answer to the supposed dilemma: Reasons and reasoning make an opinion expert, otherwise it is mere opinion. Contrary to all her accusations, the classical document examiners considered qualifications only to be qualifications to offer an opinion, not reasons for accepting an opinion. If one reads the cases she cites, one will see that is precisely why these classical experts gained such high regard.

(j) I was taught Scholastic philosophy in which one begins each question with definition of the words one will be using and review of what others have said on the question. This author rarely defines her terms, but footnote 43 on page 1741 shows why that lack is almost a virtue, since her definitions cast more shadow than light: "I am here, and throughout this paper, using 'reliability' in a legal or lay sense rather than a scientific one. By reliability I mean worth relying upon, worth crediting. Scientists use reliable to mean that a technique achieves consistent results rather than valid ones; my use of reliability is closer to what scientists refer to as 'validity.'" First, defining a term by using a form of it is not to define it. Second, the "lay" and "legal" usage of "reliability" had better be two different things. She might purchase a Webster's Collegiate Dictionary and a Black's Law Dictionary and find out what she ought to mean. Or at least try the library where she teaches. Third, I am sure even scientists will not say a technique that consistently gives poor results is reliable. That is why they calibrate their instruments so that they obtain not just consistent but also correct results. "We have reliable results, our thermometer consistently says boiling water at sea level is zero degrees Celsius!" So, since her definition of "reliability" is not valid in any usage, how can it equate to anyone's use of "validity"?

(k) At page 1742, she returns, per usual, to a previous contention, that there was a predisposition in judges to accept what they deemed reliable evidence, so handwriting experts had an easy time gaining judicial credulity by cleverly meeting the judges' "notions of what constituted persuasive proof." Utter nonsense! The historical literature shows that judges, some of whom were mentally fossilized as to inadmissibility of handwriting expert evidence, generally had to be nudged, pushed, drug, cajoled, persuaded, and imposed upon to look in the light. In one case the judge even refused to look at the physical evidence in proper daylight. See: Katherine Applegate Keeler, "Documentary evidence involved in an election dispute." 27 Journal of Criminal Law, Criminology and Police Science, 249-62 (1936). Albert S. Osborn in more than one place describes the campaign to put proper lighting in courtrooms so that physical evidence could be better demonstrated against the hidebound resistance of a few judges. If one set out tomorrow to write a history of document examination in America and does not come across these reports, one is academically inadequate. If one comes across them and chooses to omit them when they are relevant, one is academically dishonest.

(l) At page 1747: "'[R]liability' must itself be historized..., and we should be wary of assuming that our criteria are somehow either transhistorical or even clearly superior to those that persuaded past interlocutors of legal evidence." Two observations on this passage, which I mercifully truncated. First, academic writers should learn to write English, but the virtue of this horrendously awkward English is that it reduces the number willing to suffer through the article. Second, at least she seems to suspect that her overly worded text is a lot of malarkey. Which it is, as I hope my reader has at least suspected by now.

(m) She reviews some famous cases. Please, before crediting or discrediting her journalism do read the actual case reports. A caution I offer on all writing and testimony by the handwriting critics and anti-expert experts.

(n) At pages 1756-1757: "There has been only very limited study of whether self-professed experts can identify handwriting more successfully than lay witnesses." The footnoted authority is the Risinger, et al., "Exorcism" paper. These "scholars" only have themselves for authority, but as more of them repeat what they all say, they build up a "scholarly literature" which gives the illusion that there was something more than opinionated assessment as the initial "authority" for it all. Note use of "self-professed," one of the many snide insinuations of inferiority and inexpertise which infect almost every paragraph of her paper. It is like being in a room of marijuana smokers when you would never smoke the stuff. Off guard and you will ingest more than you would by smoking it yourself. So her continual use of insulting terms and the subjectively driven negative portrayal of her subjects are as contagious to the unwary mind as any flu virus is contagious to the careless bystander.

(o) At page 1774 begins a segment titled: "E. The Elimination of the Rule Against [expert] Comparisons [of handwriting]." She is unaware that this segment disproves two of her related theses, that handwriting experts campaigned to have courts change the rules and that courts drove the campaign. She must either remain blind to what facts she has correct or admit she had it wrong all along. She never alters a single thesis in the least bit. If one reads history of handwriting expertise by handwriting experts, one is struck by the better way in which these experts say the few things that she and other critics have right and say it more accurately and more succinctly.

(p) At page 1781 begins a segment titled: "F. The Use of Quasi-Experts." Her one creative idea, which I say is creative only because I have not read it elsewhere and so give credit when it is finally possible to do so, is that self-professed handwriting experts inveighed against the quasi-experts, such as bank clerks, because the latter only relied on their experience and not scientific learning and method. Now, she claims, self-professed handwriting experts insist it is their experience that demands acceptance since they have been exposed as lacking scientific knowledge and methods. It is a highly flawed idea, but rather creative and clever.

(q) At pages 1783-1784, she says, regarding only admitting percipient witnesses to writing, quasi-experts "may have" created an inequity, since a bank "could" use its own clerk, while the other party "could not" have its own bank clerk and so "would be" stuck with lay witnesses, and "this imbalance may have" prompted judges to change the rules. The later conclusion after similar arguments is: "The quasi-experts thus helped pave the way for the reception of expert testimony." Notice that a total lack of proven facts combined with two possibilities and three subjunctives supports her categorical conclusion. These law-professor critics use this logical trick more than any others, precisely because their theories have no basis in reality and no support in rationality. No merely logical argument from the subjunctive, especially an argument so contrary to the rules of logic, can prove an indicative, categorical conclusion.

(r) She takes every trait that supports the thesis of scientific practice by the classical examiners as direct evidence of unscientific practice. This is just one of many examples possible. At page 1787: "Aspiring handwriting experts thus drew upon the arsenal of scientific methods, but equally important, they invoked the rhetoric of science to buttress their own authority." [Emphasis in original.] Ignore for now the snooty way it is stated, "aspiring" and so on, so as to insinuate the conclusion will be correct. Is not using the "arsenal of scientific methods" even today a trait of scientific practice? And must not one necessarily use the terms of science when speaking scientifically? She "proves" them unscientific precisely because of the evidence that they were being scientific. Can there be a more academically flawed argument than this? She fails to do the hard work her thesis requires: demonstrate that the scientific methods were misapplied and the terms misused. As a matter of fact, the classical handwriting experts contributed to scientific methodology and instrumentation, a contribution enriching science across the board, such as Osborn's development of a better microscope and the contributions they all made to advancing scientific photography. The author of the most authoritative work in forensic photography was an old-time document examiner. See: Charles C. Scott, Photographic evidence; preparation and presentation. 2nd ed. St. Paul, MN, West Publishing Co., 1969. (3 v. with supplement up-dates.) Scott, now deceased, was a living bridge between the classical examiners and the current generation.

(s) Then at page 1788, the paragraph continuing from the previous page clearly shows her thesis entirely wrong, yet she ignores that classical examiners asked to be credited because they could show and explain the reasons for their opinions, and they did. What mental block condemns her to maintain the blind faith in the Risinger myths and to insist on blinding others to any contrary reality by adopting a most unscholarly approach to scholarship? I pity these critics so insistently, almost needfully, blind to facts. But I pity more the victims who will suffer depredations from forgers because of these slick and suavely stated fallacies.

(t) In the segment, "A. Science, Methods and Objectivity," beginning at page 1788, she reviews the early literature of American document examination. Her snide vocabulary comes fully to the fore, but staying well in the background is any capacity, if there be a capacity, to understand handwriting as a physiological and behavioral product. She does not know that handwriting qualities, like her negative literary qualities, defy quantification in many ways. We can count up her uses of denigrating terms, but only one passage so infested would be enough to show her bias as a reporter, her inadequacy as a researcher, her superficiality as a thinker, and her less than freshman quality as a lady of letters. The mass of them add nothing to the evidence.

(u) As an example of the fact that she refuses to credit the early experts even when one trumps her and her co-critics on their own major issue, is this at page 1795: "Ames, for example, thought that the real problem with expert testimony was that courts were too lenient in qualifying experts." She ought to have knelt down in humble acknowledgment that Ames defined the critical problem well before she was born, much less considered the problem which her paper never states precisely until she quotes Ames. Academic integrity would then go on and credit the solution Ames offered: Careful scrutiny of qualifications of an expert who will offer demonstration of underlying facts and clarity in explaining reasons for the opinion with application of sound reasoning. A solution better than what all the critics have cumulatively proposed.

(v) At page 1804 she is in the midst of describing fiction as if it were evidence for her thesis. A novel makes fun of every aspect of a handwriting expert. And, just as the first premise of the "validity" of the critics' position is misreading and/or misperception and/or misrepresentation of reality, so the ultimate premise is mockery. Both premises speak as to the smallness of their minds.

(w) I pass up 2 or 3 dozen other notations on the egregious errors of her intellectual ways and jump to the last two pages, 1844-1845. For page 1844, my notations say:

> "With the right twist, trimming and cramming, any fact can be made to fit any theory."
>
> "How in the h--- do we see such? She takes every opportunity to jump to her silly preconceived conclusion. Conclusion drives 'evidence' which gives excuse for conclusion."
>
> "Reconfigure [handwriting evidence]—No! Tools improve, but graphic motor sequence remains same since first writer wrote."

Then her last sentence is: "At this very moment, judges and experts are once again determining whether and how to write a new script for expert testimony in handwriting." Events, since 2001 as well as before, clearly prove this statement wrong. Cases at the appeal level endorse traditional handwriting expertise and cite historical cases, as if previous judges were not such dunces as the critics imply. Meanwhile, handwriting experts are sticking to their guns. Even the research coming out of the handwriting camp, which in this paper I do not hesitate to criticize, is in support of traditional practices, not seeking to invent new ones. So it is fitting that her last sentence is the most factually flawed in her entire, factually flawed paper.

C. WE ANTI-EXPERT EXPERTS ARE REALLY EXPERT. REALLY WE ARE.

But how good are these anti-expert experts anyway? In the most famous and initial case of attacking handwriting expertise, associates of Risinger in U.S. v Starzecpyzel, 93 Cr 553 (LMM),

880 FS 1027 (S Dist NY 1995), could not keep the Government's trial expert from testifying. However, Robert J. Phillips, in "A case report: Scott Doe et al. v. Kohn, Nast & Graff," 17 National Association of Document Examiners Journal, 28-33 (Spring 1995), describes how this same well known handwriting expert was barred from testifying on two issues of fact in the same case due to legal and technical objections to his proposed testimony. Whereupon Defendants settled. Phillips and a colleague had been consulted by Plaintiff's counsel who used the references provided. Moral: If you want to disqualify a questionable examiner of questioned documents, you need another document examiner who has mastered the literature of the field, who knows both its reliability and the limits to its reliability, who recognizes the difference between opinions of retained experts and opinions of hired experts.

See Appendix B for a review of cases where the anti-expert experts appeared. Also, for replies to the anti-expert experts, see the following publications which are a very small sampling of the many available:

1. American Board of Forensic Document Examiners. *Resource kit*. 1997.

This is an excellent collection of briefs, transcripts, articles and critiques. The document examiner would do well to obtain and study this material. An example of the excellent material Resource Kit contains is the "Errata" to the Exorcism article, written by document examiner, Charles C. Scott, now deceased. It is five single-spaced pages listing mistakes in legal citations and in legal representations. Occasional errors of this type are expected in any human production, but it is poignant evidence of unreliability and academic slovenliness that there is such a great number of them in a paper authored by three law professors, published in a prestigious law school journal, and presumably proofed and peer reviewed by editors who were either lawyers or law students. The offices of ABFDE said they are preparing a second edition of Resource Kit. It is highly recommended that you acquire this masterful compilation which I have found to be of great value.

2. OTHER REFERENCES.

(a) Fisher, M. Patricia, "A guide to the *Daubert* hearing in *U.S. v Starzecpyzel.*" 4 *Journal of Questioned Document Examination*, 3-13 (Fall 1995; and also: 5:20-32, Spring 1996; 27-42, Fall 1996).

(b) Galbraith, Oliver III, et al., "The principle of the 'Drunkard's Search' as a proxy for scientific analysis: the misuse of handwriting test data in a law journal article. The re-evaluation of results from a handwriting proficiency test program." 1 *International Journal of Forensic Document Examiners*, 7-13 (Jan.-March 1995).

(c) Harris, John J. "Author's introduction: How much do people write alike?" 5 *Journal of the American Society of Questioned Document Examiners*, 88-95 (Dec. 2002)

This is a reprint of Harris' article from 48 Journal of Criminal Law, Criminology and Police Science, 647-51 (March-April 1958), with a new introduction explaining how the article was misunderstood and/or misrepresented by Risinger, et al., in their Exorcism article and, quite persistently, elsewhere.

(d) Matley, Marcel B., *Studies in questioned documents, Number Two*: "In the Exercise of Ignorance: Replies to the Critics of Handwriting Expertise. Second, enlarged edition." San Francisco, CA, Handwriting Experts of California, 2000.

D. LORD KENYON AND THE MAKING OF THE COMMON LAW RULE.

The Common Law rule was that "comparison of hands," that is, expert opinion on authorship of handwriting, based not on personal knowledge, but on post factum examination of questioned and exemplar signatures, was of a low order of proof, and generally not admissible. When comparison by witness or trier of fact was permitted, only exemplar writings on documents otherwise admitted into evidence could be used for comparison. How did such a rule come about? There seems to be a particularly primary author of it: Lord Kenyon.

The General Index to Howell's English State Trials, covering late Seventeenth Century, cites cases wherein "Evidence of Handwriting by a comparison of writings is not admissible." However, in the late Eighteenth Century it is said that English civil courts admitted expert handwriting opinions. This according to Folkes v. Chadd, 3 Doug 157, 99 ER 589 (1782), affirmed 3 Doug K.B. 340, 99 ER 686 (1783). In 99 ER 589, at page 590, the Court stated: "On the motion for the new trial, the receiving Mr. Milne's evidence was not objected to as improper; but it was moved for on the ground of the evidence being a surprise; and the ground was material, for, in matters of science, the reasonings of men of science can only be answered by men of science.... Under persuasion of this being right, the parties go down to trial again, and Mr. Smeaton is called. A confusion now arises from a misapplication of terms. It is objected that Mr. Smeaton is going to speak, not as to facts, but as to opinion. That opinion, however, is deduced from facts which are not disputed...."

In explaining how the opinion of men of science was relied on in various fields, the Court gave this instance: "I cannot believe that where the question is whether a defect arises from a natural or artificial cause, the opinions of men of science are not to be received. Handwriting is proved everyday by opinion; and for false evidence on such questions a man may be indicted for perjury. Many nice questions may arise as to forgery, and as to the impressions of seals; whether the impression was made from the seal itself, or from an impression in wax. In such cases I cannot say that the opinion of seal-makers is not to be taken." [Emphasis added.]

Before the century was out, "every day" would become "hardly ever, if at all."

In Allesbrook v Roach, 1 Esp R 351 (1795), Lord Kenyon said he always let comparison of handwriting in. Yet in two previous cases he had and he had not. In Goodtitle ex dem. Revett v Braham, 4 Term R 497, 100 ER 1139 (1792), Lord Kenyon permitted comparison of handwritings on separate papers. Two inspectors of franks acted as handwriting experts. On the contrary, in Stranger v Searle, 1 Esp 14, 170 ER 265 (1793), Lord Kenyon ruled that an inspector of franks could not be shown genuine writing just so he could testify as to comparison with the disputed. In Leader v Barry, 1 Esp 353, 170 ER 382 (1795), tried before Lord Kenyon, a bill was proved by one of Plaintiff's witnesses to have been made by one of his clerks. It is not said whether it was a lay or skilled witness.

In Dacosta v Pym, Peake Add Cas 144, 170 ER 224 (1797), Lord Kenyon said it was best to rely on those who know the purported author's handwriting rather than the jury. "The jury, nevertheless, compared the different signatures." On the other hand, in MacFerson v Thoytes, Peake 29, 170 ER 67 (1799), Lord Kenyon said that comparison of hands is not evidence. It was reported that in Cary v Pitt, Peake Ev 154, Peake Ev. Appx 26, 84, 4th ed., Lord Kenyon rejected expert handwriting evidence. In Garrells v Alexander, 4 Esp 37 (1801), Lord Kenyon did not permit a lay witness to compare the disputed signature on a bill of exchange with that on the bail-

bond in the cause, which he had seen signed. He was permitted to say they were alike. That it was to permit the results of comparison of the bill with his recollection of the bond seemed to strike His Lordship as not the least bit odd.

Other judges accepted Lord Kenyon's final flip as settled authority. For example, in The King, on the Prosecution of Jackson, v Joseph Cator, 4 Esp 117, 170 ER 661 (1802), an inspector of franks was not permitted to testify from comparison of hands, the judge citing Lord Kenyon's ruling as an authority. One can reasonably infer that, if such had not been permitted with some frequency, the barrister would not have offered it. It would be an interesting study in the psychology of case law to find out why one and not another of his several altering positions became enshrined in English Common Law.

Thomas Peake, in A Compendium of the Law of Evidence, American reprint, Philadelphia, 1812, states that mere comparison of hands was admissible when no other means of proving the handwriting was available, since it was the best evidence allowed in the circumstances. Thus the rejection of expert evidence as to handwriting even in English civil courts was never total. The inspectors of franks mentioned in the several cases above were employed by the Postal Service specifically to detect forged franks, signatures used in lieu of stamps by those privileged to send mail without paying postage, and so a professional coterie of handwriting experts existed in England during the entire time that Risinger, et al., claim none did. As stated elsewhere, Ecclesiastical Courts of England, having by statute jurisdiction in some civil matters which were appealable all the way to the House of Lords, always admitted expert handwriting opinions. If, as Risinger and his like say, no handwriting experts existed in England of that time, were they imported for each case or did judges raise from the dead those long buried who had done the work under Emperor Justinian's Code which permitted it? Obviously, the profession was alive and well.

E. COMPETENCY TESTING; RATE OF ERROR TESTING.

There have been published studies of competency testing and rate of error in handwriting expertise, but several were intended to test other aspects of document examination; however, the authors of these studies did not advert to the dual nature of their studies nor have "scholars" on both sides of the current debate done a minimal job of literature research to locate these studies, nor are most document examiners good students of their own rich heritage of professional literature. Some selected items are offered here and in the following section. They are given chronologically.

Imbau, Fred E., "Lay witness identification of handwriting; an experiment." 34 *Illinois Law Review,* 433-43 (December 1939).

Seven professors, six secretaries, eight lay persons of various professions selected to represent composition of the usual jury, seven bankers, and three handwriting experts were given the same sets of signatures to determine which were genuine and which false. The experts had a combined accuracy rate of 90% and 96% for the two tests, while the other groups ranged from 11% to 83% for either test.

Widacki, J., and F. Hovath, "Experimental investigation of the relative validity and utility of the polygraph technique and three other common methods of criminal identification." 23 *Journal of Forensic Sciences*, 596-601 (July 1978).

Conducted at a Polish University, this seems to be the same research project as reported in the next item. It is cited and discussed in footnote 24 to the dissenting opinion by Justice Stevens in U.S. v Scheffer, U.S. Supreme Court, No. 96-1133, Decided March 31, 1998, reversing 44 M. J. 442.

Widacki, J., and F. Hovath, "Experimental investigation of the relative validity of the polygraph technique and three other common methods of criminal identification." 7 *Polygraph*, 215-21 (Sept. 1979).

This seems to be a reprinting of the previous item. Steve VanAperen says of it in Investigative News and Articles, March 22, 2004:

"In 1978 a study by Drs. Hovath & Widacki was conducted which compared the polygraph with handwriting analysis, eyewitness identification and fingerprints. A group of 80 students was divided into 20 groups of four persons. A mock crime was then committed by one student in each group and the abovementioned investigative means were employed by experts in each field (in the case of the eye witnesses, they had been told they would be eyewitnesses to a crime prior to the experiment) to try to correctly identify the offender in each group. The results were as follows:

Identification	Correct	Incorrect	Incon.
Polygraph	18	1	1
Handwriting	17	1	2
Eyewitness	7	4	9
Fingerprints	4	0	16"

The full data is more rich and complex than the above, but this serves to illustrate a high rate of accuracy in handwriting identification.

Kam, Moshe, *et al.*, "Proficiency of professional document examiners in writer identification." 39 *Journal of Forensic Sciences*, 5-14 (January 1994).

Blake, Martha, "Are we seeing the same thing? Results of a survey presented to forensic document examiners. A pilot study to validate handwriting comparisons." 1 *International Journal of Forensic Document Examiners*, 32-9 (Jan.-March 1995).

Much work in designing, sending out, comparing and analyzing results statistically went into "an attempt to validate handwriting companion." The last sentence says: "This survey is a small step in that direction." [Emphases added.]

Dawson, Greg A., and Brian Lindblom, "An evaluation of line quality in photocopied signatures." 38 *Science and Justice*, 189-94 (1998).

In testing whether photocopies would sufficiently indicate line quality, the authors indirectly provided compelling evidence of competence in 72 examiners from Australia, Canada, Great Britain and United States. At page 191: "The overall line quality characteristics in 69 of 72 tests

(95.8%) were accurately assessed in the photocopies representing 33 of 35 (94.3%) genuine and 36 of 37 (97.3%) non-genuine signatures. In this same 69 of 72 responses, similarities and differences in line quality characteristics were accurately distinguished between the photocopied questioned signature and the known samples." If the examiners had not been very competent and skilled, they would not have been so accurate and reliable in assessing less than ideal material. Since results were uniform across international participation, there must not have been any notable differences in method and skill among participating examiners from the four countries.

Found, Bryan, *et al.*, "The development of a program for characterizing forensic handwriting examiners' expertise: Signature examination pilot study." 2 *Journal of Forensic Document Examination*, 69-79 (Fall 1999).

A group of experts and one of lay persons had equal number of correct identifications, but the lay persons made more errors in saying simulated signatures were genuine. The experts were far more conservative and gave far more inconclusive opinions.

Kam, Moshe, *et al.*, "Signature authentication by forensic document examiners." 46 *Journal of Forensic Sciences*, 884-8 (2001).

Sita, Jodi C.. *et al.*, "Forensic handwriting examiners' expertise for signature comparison." 47 *Journal of Forensic Sciences*, 1117-24 (Sept. 2002).

Reliability was demonstrated, but it had no significant correlation with years of experience.

Found, Bryan, and D. K. Rogers, "Investigating forensic document examiners' skills relating to opinions on photocopied signatures." 45 *Science and Justice*, 199-206 (2005).

F. ERRORS IN HANDWRITING OPINIONS.

Much has been made about lack of rate of error studies in handwriting identification. Years before most critics probably knew about the concept of "rate of error," handwriting experts had done a far better thing. This was to identify the sources of error, thus bringing about a direct and indirect benefit to courts of law. The direct benefit was the intended one, to provide the examiner of handwriting with a checklist of things to review in order to avoid errors in opinions. The indirect benefit, probably unintended, was to provide an astute cross-examiner with the finest tool for exposing the causes of error in handwriting opinions. If a cross-examiner asks the witness about each source of error and the examiner cannot say that factor was covered, then a lack is shown in the care with which the work was done and the final opinion reviewed. Knowing that in general a field of expertise has a rate of error of only 5% does a cross-examiner no good unless the same study tells how to find out whether this expert is among the 5% or the 95%. To say such a rate of error means one can have that much confidence in this expert's opinion is to be one of those born every minute.

Here is a selection of papers related to error in handwriting opinions.

Anonymous, "Experts in handwriting and their blunders." 3 *Law Notes (Northport, NY)*, 211-2 (Feb. 1900).

Beck, Jan, "Sources of error in forensic handwriting evaluation." 40 *Journal of Forensic Documents*, 78-82 (Jan. 1995).

Kingsley, William J., "Handwriting experts and the Broughton case." 17 *Banking Law Journal*, 599-602 (August 1900).

At page 599: "That the newspaper writers who criticize handwriting experts for inaccuracies are far more inaccurate about their own work is demonstrated by the woefully jumbled up statements of this Broughton case." The newspaper reports had almost all substantive facts wrong. Maybe being a law professor and anti-expert expert calls on the same expertise and objectivity as being a newspaper reporter in 1900.

Osborn, Albert S., "Errors in identification of handwriting." 48 *American Law Review*, 849-58 (Nov.-Dec. 1914).

Osborn, Albert D., "Errors in handwriting identification." 22 *Connecticut Bar Journal*, 385-9 (December 1948).

Williams, Montagu, "Experts in handwriting." 25 *American Law Review*, 967-9 (Dec. 1891).

Charles Cabot and Frederic George Nethercliffe testified that the accused had written an anonymous post card. Then the true author testified to its making.

There are others, and also books will treat the topic. The above sampler should suffice as a starting point for someone wanting to research the matter.

G. INDIVIDUALITY IN HANDWRITING.

This is a third area in which it is falsely claimed no studies had ever been published. Both the alleged scholars who have waded in on either side of the debate and many document examiners have shown great sloth or ineptitude in not discovering such materials readily available in academic libraries. The scholars might be excusable in this lack of scholarship in that most if not all are professors whose communally practiced and approved method of research seems to be to have students do it all. Since such scholars, who appear to specialize in interpretations and pronouncements versus verifiable facts, seem to be hidebound in their initially chosen stance on any given dispute, one suspects that the students' grades depend on how conforming the assigned research results are to the professor's a priori conclusions. So the professor, by following accepted procedures in his academic milieu, might only grow ever more assured of his original and unflinching assurance.

The citations are given chronologically in order to emphasize those predating Daubert.

Conway, James V. P., *Evidential Documents*. Springfield, IL, Charles C. Thomas Publisher, 1959, third printing 1978.

The author describes several cases in which, by a few individualistic traits, a writer was identified from among a number of potential suspects. In the first case on pages 41-43, an extortionist was identified among 70 sixth graders in the same class. The writer was the only one with the six key individualities determined from a study of the extortion letter.

On page 43 a case is described wherein the writer of obscene letters was identified from among 20,000 young military men. All personnel on the base were required to fill in a questionnaire designed as comparison material with the obscene letters. A rapid scanning of the questionnaires determined the writer who confessed.

On page 43 the author reports that the kidnap-murderer Angelo LaMarca was identified by a mass search among writings by an estimated 2,000,000 persons in the New York area.

On pages 43-44 the case is given of a writer of anonymous letters who was discovered out of a large metropolitan population because of handwriting identification.

No citations are given to prior publications of these cases.

Livingston, Orville B., "Frequency of certain characteristics in handwriting, pen-printing of 200 people." 8 *Journal of Forensic Sciences,* 250-9 (April 1963).

Using cards filled out by individuals who were arrested, 81 traits are illustrated by which cards were classified and sorted. The percentage of writers using each particular trait is given. The author describes how the cards were selected or rejected for analysis. The study employed a standard card sorting method by which a needle holds some cards and lets others fall, depending on traits found in the writing represented by whether a hole punched at the card's edge was left intact or cut away. The system permitted narrowing a search to suspects sharing traits found in a questioned writing until a single culprit could be identified. Without the author's stating it, the method confirms the thesis that it is a combination of significant traits reasonably expected to be found in most if not all handwritings by the same individual that permitted identification of a writer.

Harvey, R., and R. M. Mitchell, "Nicola Brazier murder; the role of handwriting in a large-scale investigation." 13 *Journal of the Forensic Science Society*, 157-68 (July 1973).

Out of 1,046 sample writings by men of various ages and occupations, only one had a combination of several specific traits considered potentially identifying by study of the questioned writing before the large-scale handwriting investigation began.

Muehlberger, Robert J., "Statistical examination of selected handwriting characteristics." 22 *Journal of Forensic Sciences,* 206-15 (Jan. 1977).

Initial "th" digraphs in writings from 200 individuals were studied as to six features only. Statistical tables give the incidence of occurrence for a total of 25 variations under the six features. By even such modest observations and results, it seems none of the 200 had the same combination of variations for the six features; however, the purpose of the study was to show how to obtain an estimate for the frequency of traits in the general population.

Baxendale, D., and I. D. Renshaw, "Large scale searching of handwriting samples." 19 *Journal of the Forensic Science Society*, 45-51 (1979).

Police sought the writer of notes found in a stolen car in an abduction and murder case. Experts selected a handful of traits to be accepted and a handful to be rejected by non-experts who surveyed public records. After 600,000 records were surveyed, the guilty party was positively identified out of all suspects submitted through a more thorough comparative analysis by experts.

Cheung, Y. L., and Sze Chung Leung, "Identification by statistical classification techniques. A comparative approach to the examination of Chinese handwriting, part 4." 29 *Journal of the Forensic Science Society* (77-89, March-April 1989).

This is one of a five-part series which offers statistical analysis of how common or rare certain features are in Chinese handwriting. Each research study in the series had more than 400 subjects. The five studies validated principles of identification, most of which are the same as used in examining occidental handwriting. Also, special principles used in examination of Chinese writing are directly applicable to other systems based on the Chinese.

Fisher, David. *Hard evidence; how detectives inside the FBI's sci-crime lab have helped solve America's toughest cases.* New York, Simon & Schuster, 1995.

On page 196 he discusses the LaMarca case Conway discusses, but he gives the name as John LaMarca. This is followed by a discussion of the prosecution of Michele Sindona for bank fraud. While trial was going on the suspicion arose that he had flown to Italy and back to New York under a fictitious name, when he supposedly was being held by kidnappers. This lead an FBI agent to spend days examining all available customs forms filled out during the return flights from Italy to New York for a two-week period. Only one card out of many thousands showed a dot inside the bowl of handwritten number nine, a writing habit of Sindona. A partial fingerprint confirmed the authorship. Sindona had lied about the kidnaping, a fact that helped persuade the jury to convict. All from one rare, idiosyncratic graphic trait.

Horton, Richard A., "A study of the occurrence of certain handwriting characteristics in a random population." 2 *International Journal of Forensic Document Examiners*, 95-102 (April-June 1996).

1,700 forms were distributed to military and civilian personnel of the U.S. Army, and 580 forms were returned and statistically analyzed. Several preselected traits were analyzed as to statistical occurrence in the subjects' samples.

Tull, Pat. "A study of numbers, their variation and variability; a contemporary study of the variation in the construction of numbers and a comparison with two previous studies." 10 *Journal of Forensic Document Examination,* 41-51 (Fall 1997).

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Vastrick, Thomas R., "The uniqueness of handwriting." 1 *Journal of the American Society of Questioned Document Examiners*, 4-7 (June 1998).

The author reports a survey of 91 document examiners with a total of 1,490 years of experience as to whether any had ever encountered writings by two individuals which lacked significant difference between them. All answered no. He then surveyed published and unpublished, empirical and statistical studies on the individuality of handwriting. This survey includes many papers that review handwriting of specific groups and either directly or indirectly support the thesis of individuality in handwriting. Vastrick's paper is an especially valuable contribution to the literature as a bibliographic source.

Welch, John R., "A review of handwriting search cases as an indicator of the individuality of handwriting." 5 *International Journal of Forensic Document Examiners*, 283-4 (Jan.-Dec., 1999).

The author reviews four cases, two of which are reported in papers by Harvey/Michell and Baxendale/Renshaw discussed above. The third case was the Yorkshire Ripper. Six characteristics were selected from an anonymous writing which police believed was from one who had personal knowledge of the crimes. The six characteristics proved most of 100,000 writings compared to be by different individuals, and of those that had the six characteristics a closer examination eliminated them. Apparently the anonymous writer was not identified, but large numbers of possible suspects were positively eliminated.

The fourth case had to do with theft of government information. Five characteristics were selected, three of which had to be found in a suspect's writing for the writing to be given a closer examination by experts. The pool of potential suspects was 10,000. After 1300 handwritings had been screened, the culprit was positively identified by the handwriting experts and subsequently confessed. Note that in these mass screenings, only a handful of traits, not the most significant but ones easily observed by non-experts, were selected. The combination of these with a follow-up of a full handwriting comparison was what made identification eventually possible. Critics, misunderstanding basic identification theory, fail to realize that it is a unique combination of characteristics significant for identification that is required to determine who a writer is, and thus no single feature need be unique to any given individual, nor need the statistical occurrence of the features relied on be known.

Crane, A. "The frequency of round handwriting in Edmonton, Alberta schools." 32 *Canadian Society of Forensic Science Journal* (Dec. 1999).

Ahola, N. M. "Classification and frequency of occurrence of specific number styles." 33 *Canadian Society of Forensic Science Journal* (March 2000).

Srihari, Sargur, "Individuality of handwriting." 47 *Journal of Forensic Sciences*, 856-72 (2002).

A lot of cheering has greeted this study from those in handwriting examination and a lot of boos from the critics. It is far from being as good or as bad as the two camps see it since the

researchers did not seem to understand the dynamics of handwriting any more than the critics or the inadequate examiners do.

Harrison, Diana and Danielle Pettine Seiger, "Meeting the *Daubert* challenge: a bibliography of handwriting articles for the forensic document examiner." 5 *Forensic Science Communications*, #1 (January 2003). [An online journal from the FBI with free access.]

Papers listed are published journal articles and presentations at conferences. No information is provided as to how to obtain the latter. The papers either directly or indirectly support individuality in handwriting. The published papers considered directly supportive are included in this bibliography.

H. BUT IT IS NO EXCUSE!

Even if all the world woke up tomorrow in agreement that every individual, from the moment when one can pick up pen and put it to paper, wrote a thoroughly and uniquely identifiable handwriting, the expert at court ought still be required to do the expert thing in support of an opinion. To summarize what has been discussed previously in this paper:

1. The expert must prove what in this disputed handwriting makes it distinctly identifiable as coming from its specific author and no other person.
2. Next the expert must prove what in the exemplars of the suspected writer ties each and every exemplar to their common maker and what will prove any new writing by the same person as being by that person and no other.
3. Next the expert must prove that, among all those comprising the population of possible makers of the disputed writing, none other than the one identified has the specific complex of identifying traits established for either the writing in dispute or the exemplars of the person identified as the maker of the disputed writing.
4. The expert should be able to say which of the twelve factors accounting for individuality in handwriting are operative in the instant case.
5. The expert should be able to explain how each source of error has been avoided and how each limitation in the examination was either overcome or compensated for or taken into consideration in expressing the certitude of the opinion.
6. Along the way any factor negating the opinion must be given a reasonable explanation or the opinion must be either qualified or abandoned.

These six points are not the whole of the matter, but they surely are among the minimal essentials. Yet many handwriting experts are mostly expert at escaping the obligation of satisfying them.